ON GROWTH AND FORM IN GEOMORPHOLOGY

NICK SPEDDING
Department of Geography, University of Aberdeen, Elphinstone Road, Aberdeen, AB24 3UF, UK

Received 15 March 1996; Revised 23 May 1996; Accepted 15 August 1996

ABSTRACT

Perhaps surprisingly, geomorphology's relative failure to deliver meaningful process-based accounts of landscape development has not stimulated much in the way of procedural debate. Although most geomorphologists seem to agree that a problem exists – how best to make explicit the links between process and form? – this tends to be seen as a substantive problem only, the solution to which lies within the existing framework of geomorphic research, located broadly within the tradition of positivist scientific method. Here I argue that we need to ask a new type of question in a new way: one which gives priority to organizational/compositional relationships rather than to detailed process studies, within the revived context of space–time dynamics. Such a framework draws loosely on complexity theory and realist philosophy, and, in the first instance at least, suggests a return to conceptual, qualitative methods of research. © 1997 by John Wiley & Sons, Ltd.

Earth surf. processes landf., **22,** 261–265 (1997)
No. of figures: 0 No. of tables: 0 No. of refs: 32
KEY WORDS: geomorphology; biology; reductionism; realist philosophy; context; complexity

Standard histories of geomorphology tend to identify a basic conflict between two extremes: (1) (timebound) descriptive regional studies of landscape evolution, and (2) the (timeless) analysis of process mechanics which emerged in the post-war years to challenge the hegemony of its predecessor. This dichotomy represents a pervasive myth within geomorphology which perpetuates the impression that any study must choose between the historical or ahistorical category, and its associated package of research methods, scales of analysis, etc. To give just one example: the special issue of the *Journal of Glaciology* published to mark the fiftieth anniversary of the International Glaciological Society includes individual papers which separately review the histories of the scientific investigation of glaciers (Clarke, 1987) and glacial geology (Boulton, 1987). I take this as tacit endorsement of the view that the study of the behaviour of ice stands apart from the study of its concrete time- and place-specific impacts. The legacy of such divisions is seen today in the growing insistence that geomorphology must put itself back together by means of the explicit study of process–form relationships which integrate the mechanistic and the historical, e.g. within glacial geomorphology see Harbor (1993) and Menzies (1995).

If we are to accept that the relative failure of geomorphology to deliver meaningful process-based accounts of landform/landscape development represents something of a crisis, what is surprising is the complacency with which the discipline has let the crisis develop. I think it is fair to say that a large number—perhaps a majority—of professional geomorphologists subscribe to the rhetoric which advocates a redefinition of geomorphology as the explicit study of process–form linkages. If this represents a genuine sense of collective unease, standard accounts of the history and sociology of science might lead us to expect a period of intense debate among geomorphologists as we search for new ways to meet this new challenge. However, signs of such debate are scarce.

This lack of self-reflection can perhaps be explained by the suggestion that although geomorphology admits to a substantive research problem – how best to bridge the gap between process and developing form? – it refuses to see this also as a methodological problem. The fact that the majority of the participants at the BGRG Strathclyde symposium chose to interpret its theme as an invitation to present/discuss substantive issues seems

CCC 0197-9337/97/030261–05

to support this argument. Within the discipline, the publication of a conceptual paper is a rare event: writing in 1978, Chorley noted that 13 years had passed since a paper of major theoretical significance (Schumm and Lichty, 1965) was last published! ‘Classics’ such as this, or Wolman and Miller’s (1960) work on the magnitude and frequency of geomorphic events, are rehearsed diligently to introductory undergraduate classes, but make little impact on contemporary research. In particular, geomorphology as a discipline tends to distance itself from debates which touch on the philosophy of science. It is common to regard philosophy as an irrelevant diversion from the pragmatic business of research (Chorley, 1978, p. 1; Bassett, 1994; Frodeman, 1995). As a result, geomorphology proceeds within the research culture to which it was initiated in the 1950s: that of ‘scientific method’. Few pause to consider the implications of this. Fewer still see fit to challenge the hegemony of scientific method as a means of solving geomorphic problems (Richards, 1994).

Geomorphology’s association with scientific method ties it to a research programme which is broadly positivist. It is difficult to be more precise, because the move away from descriptive historical studies itself created a new dichotomy between ‘empirical/functional’ studies and ‘rational/realist’ studies (Mackin, 1963; Chorley, 1978). Functional (instrumentalist) studies seek to express empirical regularities by means of ‘law-like’ mathematical statements of co-variation, whereas ‘realist’ studies try to uncover ‘law-like’ descriptions of fundamental physical processes. Descriptions of trough morphometry using parabolic or quadratic equations (e.g. Graf, 1970) provide one example of the functional approach to glacial geomorphology, whereas the ‘realist’–reductionist approach to trough formation begins with the detailed analysis of bedrock erosion processes (e.g. Hallet, 1981; Iverson, 1991). Both schools share the view, which is challenged by (‘true’) realist philosophy (e.g. Sayer, 1992), that the relationship between cause and effect is self-evident. This simple view, with its implicit, if fallacious, assumption of system closure (by which it is meant that all system variables are fixed), drives the search for regularities within a framework which is (supposedly) objective, with priority, indeed respectability, attached to the use of technology-driven experimentation, simulation and prediction, directed towards the production of quantified data and results.

Since the 1970s, the majority of studies have tended to fall within the ‘realist’–reductionist method, from which preference springs the present debate. At present, large parts of the discipline seem confident in the expectation that building within this existing framework – scaling-up increasingly refined process models – will enable us to solve the problems of landscape development. What are required are better instruments, better dating techniques, better computer models, etc. This is the ‘disciplinary immaturity’ response frequently used to explain the relative failure of the social sciences: i.e. predictive success is simply a matter of applying the scientific method with greater rigour. Harbor (1993, p. 129), for example, announces that ‘as we head towards the twenty-first century, glacial geomorphology will advance through the use of three-dimensional numerical models that include ice flow, basal sliding (with explicit consideration of deformable beds), erosion and deposition processes, and underlying material characteristics’.

As yet, the computer model which brings together all these factors in a holistic account of glacial landscape development is the subject of science fiction. However, it is not so much technical barriers to model performance which limit their utility, but conceptual constraints. Computer models, or, indeed, specific laboratory experiments or fieldwork case studies, seek to answer certain types of questions in certain types of ways. Models which isolate certain processes, and explore the impact of these, act as a conceptual prop to our understanding, but by their nature cannot provide a complete explanation of landscape development. It is important to accept the partial character of models, and not to place too much faith in their performance or potential, nor to read too much into their conclusions. For example, a computer model of glacial trough development which suggests that glaciers erode rapidly (Harbor, 1992; Harbor and Warburton, 1993) does not, in the absence of a comparable model of subaerial valley development, tell us that glaciers erode more rapidly than rivers. Similarly, improved dating techniques are not a Rosetta Stone, magically revealing the processes of landscape development. However powerful, dating techniques are *just* dating techniques: a partial approach which cannot answer questions it does not ask.

As I see it, the chief problem is that of the *context* within which geomorphology chooses to operate. Perhaps the best demonstration of this is Schumm and Lichty’s (1965) paper on time, space and causality, the essence of which is that the same behaviour (geomorphic activity within the drainage basin) looks very different depending on the temporal and spatial scales at which we look at it. If we look at long intervals of time applied to

large areas, we tend to see a sequence of historical changes as the landscape evolves; these historical states, however, tend to conceal the action of geomorphic processes. Conversely, if we study small areas for a short time, we tend to prioritize process behaviour, but lose our sense of history. Subjective choice by the scientist defines both the context – the collection of concepts and techniques used to structure the research problem – and the nature of results. Quantum physics, for example, suggests that at the sub-atomic level matter can behave simultaneously as both waves and particles. What the scientist sees depends on what he sets out to see: if he asks 'wave' questions, he gets 'wave' answers; 'particle' questions, 'particle' answers (Gribbin, 1984). Significantly, to study property A in detail implies that the scientist cannot study property B of the same thing in detail, and vice versa: this is the Heisenberg uncertainty principle. A similar effect is often seen in geomorphology: historical studies which prioritize dating and the calculation of bulk process rate (e.g. of erosion or sedimentation) rarely permit us to identify the complex space–time dynamics of individual processes. To extend the logic of this argument, the 'bigger, better, faster, more!' approach, for all the insights into individual aspects of geomorphology it is likely to give, will not necessarily produce the desired synthesis of process and landscape development because its asks a certain type of question within a certain tradition of scientific enquiry. If we are to claim the middle ground within which this synthesis lies, we must ask a new type of question which in turn requires a new context of investigation (see, for example, Kennedy, 1977; Chorley, 1978).

Recent work by Goodwin (1994), writing on the current status of biology, presents an instructive parallel here. He relates a narrative whereby biology developed its strength and identity as an individual scientific discipline by means of the widespread adoption of Darwinism. Thus the central theme of biology became that of tracing the historical sequence of changing life-forms. However, this descriptive, evolutionary approach was replaced by the rise of molecular biology in the 1950s, and the focus of research switched to reductionist investigations into the basic mechanisms of genes. With genes installed as 'the basic elements of biological reality', the status of the life-form within biology diminished to the extent that any given historical state of the organism could be explained as the product of 'the evolutionary adventures of genes'. Within this new paradigm, the exact events by which genes are linked to life-forms and their development/evolution are simply taken for granted.

The parallels here with the standard history of geomorphology are striking, even to the publication just one year apart of the iconic papers taken to symbolize the new eras in geomorphology and biology (Strahler, 1952; see also Strahler, 1992; Watson and Crick, 1953). However, yet more pertinent is Goodwin's argument that if biology is to reinvent itself as a science which provides a meaningful link between the fundamental level of genetics and the level of the organism – the study of the interactive mechanics of morphogenesis (development of form) – it must adopt a new approach. Goodwin redefines the central problem of biology as one of *organization*, drawing on the science of complexity to create a research framework which emphasizes the morphogenetic dynamics inherent to interactive systems. Form develops within a complex generative field, which reflects the influence both of immanent features and wider environmental influences. Feedback between process and form drives the system as the morphogenetic dynamic is continuously redefined in response to the developing form it itself creates. The priority attached to properties of things such as genes or basic geomorphic processes – the traditional focus of post-war research – disappears: *it is not the building blocks themselves which matter so much as the way in which they are put together.* This recalls Simpson's (1963) distinction between the reductionist, ahistorical (laboratory) sciences and the *compositional* historical sciences, chiefly geology and biology.

Given the current debate within geomorphology, the appeal of Goodwin's emphasis on the dynamics of morphogenesis is clear. Given the traditional niche of geomorphology (within the UK) as a subdiscipline of geography, Goodwin's redefinition of the appropriate context of investigation is equally attractive. It is the context of space and time that mediates the physical relationships which give rise to morphogenesis: 'What counts in the production of spatial patterns is not the nature of the molecules and other components involved . . . but the way these interact with one another in time (their kinetics) and in space (their relational order: how the state of one region depends on the stage of neighbouring regions). These two properties together define a field, the behaviour of a dynamic system that is extended in space, which describes most real systems' (Goodwin, 1994, p. 49).

To redefine biology, or geomorphology, in terms of the study of organizational dynamics is to restore its natural context, which carries within it a critique of the established reductionist tendency in science. The weakness of classic reductionist method is that it is both aspatial and atemporal: thereby it denies the complexity of the real world it purports to explain. Thus Goodwin (1994, p. 168) asserts that the 'reductionist insistence on some basic material level of cause and explanation . . . can be recognised for what it is – an unfortunate fashion or prejudice that is actually bad science'. This is perhaps too strong: Cohen and Stewart (1994, p. 3), in a similar analysis to Goodwin's, take a less extreme view: 'We think [the reductionist story] is right as far as it goes, but it doesn't go far enough'. This gives us criteria by which we can judge the utility of reductionist science: it is useful to the extent that it is able to identify the key interactions which control the trajectory of landscape development. Progress need not necessarily await further advances in our knowledge of process mechanics; instead, it depends upon the skill with which we are able to tie together critical causal factors within coherent explanatory structures. Contextual relationships replace non-contexual mechanistic relationships as the basis of explanation, at which level the fine details of process can often be taken for granted. Harbor's (1992) computer model simulates effectively the development of glacial trough cross-sections using a simple relationship between the square of basal ice velocity and the rate of erosion, which is some way removed from the abstract and in-depth mathematical analysis of Hallet (1981) but is nevertheless consistent with Hallet's reductionist formulations. Hooke (1991) draws on Iverson's (1991) quantitative analysis of quarrying to propose a *qualitative* model of glacial overdeepening which proceeds by means of a positive feedback involving ice dynamics, hydrology and topography. Similarly, Kirkbride and Spedding (1996) suggest that the presence of overdeepenings can interact with drainage and sediment transfer to establish a particular pattern of moraine formation.

To rediscover the context of space–time relationships is simultaneously to retreat from the rigorous, quantitative analysis celebrated by traditional scientific method. The switch towards conceptual, qualitative analysis, at least to begin with, is a central theme of the books by Goodwin and Cohen and Stewart. It also fits with the distinction between the positivist emphasis on explanation as prediction and control, relative to the realist's quest for explanation as understanding (Richards, 1990; Sayer, 1992). Both realism and complexity theory share an interest in 'emergent' phenomena: the pervasive reproduction of certain robust structures (landforms) within apparently diverse systems (the creation of 'order out of chaos'), the genesis of which is not immediately obvious from the study of the system's individual components. Many moraines are examples of emergent landforms. Delivery of substantial quantities of sediment to the ice margin is necessary to build a large moraine. This implies both that (1) bedrock erosion creates large quantities of debris, and (2) thereafter, the debris is entrained and retained within the ice, and not flushed beyond the glacier by the action of meltwater. Variance in moraine size cannot be partitioned between the two factors, with, say, 50 per cent of the variance attributed to debris production and 50 per cent to debris retention. The two factors are not independent and additive in terms of their causal properties, but are intimately linked within the emergent structure which constitutes the debris transfer system of a glaciated catchment. The debris production and retention factors themselves represent the emergent product of several interdependent variables, including glacier dynamics, bedrock topography amd configuration of the drainage network (see previous paragraph).

The extent to which the study of emergence remains consistent with the analytical rigour implied by the realist emphasis on the recovery of 'ontological depth' is likely to depend on the subdiscipline of geomorphology involved. The 'new' fluvial geomorphology succeeds in retaining a strong quantitative, quasi-reductionist element *within* the perspective of a spatially and temporally distributed approach by means of its choice of a morphologically dynamic, small-scale, readily accessible study environment (frequently a proglacial meltwater stream), allied with new techniques and instruments such as photogrammetry, digital elevation modelling and the use of electromagnetic current meters (Lane, 1995). Progress in glacial geomorphology, in which process–form interactions evolve much more slowly, and are often concealed beneath the ice, is likely to be far less spectacular in this respect.

In geomorphology, as with geology, it is important to restore the respectability of conceptual, qualitative methods and so free us from the position of feeling we have to apologize for the interpretative, historical character of our science (Frodeman, 1995). These characteristics follow directly from the fact that geomorphology, if we are to define it as the process-led study of landform development, *is* a spatially and

temporally located science, i.e. it deals with an open system in the realist sense of system closure. Herein lies both its attraction and its strength. Richards (1990) and Sugden (in press) suggests that geomorphology has perhaps as much in common with the social sciences as it has with the physical sciences. Arguably, human geography became the richer because of its willingness to reinvent itself, to explore beyond its traditional boundaries and ask new questions within a contextual framework of dynamic socio-spatial organization (e.g. Dear, 1989; Gregory, 1994). Is now not the time for geomorphology to try the same?

ACKNOWLEDGEMENTS

The ideas expressed in this paper were developed in large part whilst I was in receipt of the Lamb-Meiklejohn Research Studentship at the Department of Geography, University of Edinburgh. The critical comments of Stuart Lane and an anonymous referee forced me to clarify many of my arguments.

REFERENCES

Bassett, K. 1994. 'Comments on Richards: the problems of 'real' geomorphology', *Earth Surface Processes and Landforms,* **19**, 273–276.
Boulton, G. S. 1987. 'Progress in glacial geology during the last fifty years', *Journal of Glaciology,* (Special Issue), 25–32.
Chorley, R. J. 1978. 'Bases for theory in geomorphology', in Embleton, C., Brunsden, D. and Jones, D. K. C. (Eds), *Geomorphology: Present Problems and Future Prospects,* Oxford University Press, Oxford, 1–13.
Clarke, G. K. C. 1987. 'A short history of scientific investigations on glaciers', *Journal of Glaciology,* (Special Issue), 4–24.
Cohen, J. and Stewart, I. 1994. *The Collapse of Chaos,* Penguin, London, 495 pp.
Dear, M. 1989. 'The postmodern challenge: reconstructing human geography', *Transactions of the Institute of British Geographers,* **NS 13**, 262–274.
Frodeman, R. 1995. 'Geological reasoning: Geology as an interpretive and historical science', *Geological Society of America Bulletin,* **107**, 960–968.
Goodwin, B. 1994. *How the Leopard Changed its Spots,* Weidenfeld and Nicolson, London, 233 pp.
Graf, W. L. 1970. 'The geomorphology of the glacial valley cross-section', *Arctic and Alpine Research,* **2**, 303–312.
Gregory, D. 1994. 'Social theory and human geography', in Gregory, D., Martin, R. and Smith, G. (Eds), *Human Geography: Society, Space and Social Science,* Macmillan, Basingstoke, 78–109.
Gribbin, J. 1984. *In Search of Schrödinger's Cat,* Black Swan, London, 302 pp.
Hallet, B. 1981. 'Glacial abrasion and sliding: their dependence on the debris concentration in basal ice', *Annals of Glaciology,* **2**, 23–28.
Harbor, J. M. 1992. 'Numerical modeling of the development of U-shaped valleys by glacial erosion', *Geological Society of America Bulletin,* **104**, 1364–1375.
Harbor, J. M. 1993. 'Glacial geomorphology: modeling processes and landforms', *Geomorphology,* **7**, 129–140.
Harbor, J. M. and Warburton, J. 1993. 'Relative rates of glacial and non-glacial erosion in alpine environments', *Arctic and Alpine Research,* **25**, 1–7.
Hooke, R. Le B. 1991. 'Positive feedbacks associated with the erosion of glacial cirques and overdeepenings', *Geological Society of America Bulletin,* **103**, 1104–1108.
Iverson, N. R. 1991. 'Potential effects of subglacial water pressure fluctuations on quarrying', *Journal of Glaciology,* **37**, 27–36.
Kennedy, B. A. 1977. 'A question of scale?', *Progress in Physical Geography,* **1**, 154–157.
Kirkbride, M. P. and Spedding, N. 1996. 'The influence of englacial drainage on sediment transport pathways and till texture of temperate valley glaciers', *Annals of Glaciology,* **22**, 160–166.
Lane, S. N. 1995. 'The dynamics of dynamic river channels', *Geography,* **80**, 147–162.
Mackin, J. H. 1963. 'Rational and empirical methods of investigation in geology', in Albritton, C. C. (Ed.), *The Fabric of Geology,* Addison-Wesley, Reading, MA, 135–163.
Menzies, J. 1995. 'Preface', in Menzies, J. (Ed.), *Modern Glacial Environments,* Butterworth-Heinemann, Oxford, xi–xii.
Richards, K. S. 1990. ' 'Real' geomorphology revisited', *Earth Surface Processes and Landforms,* **9**, 277–281.
Sayer, A. 1992. *Method in Social Science: a Realist Approach,* 2nd edn, Routledge, London, 313 pp.
Schumm, S. A. and Lichty, R. W. 1965. 'Time, space and causality in geomorphology', *American Journal of Science,* **263**, 110–119.
Simpson, G. G. 1963. 'Historical science' in Albritton, C. C. (Ed.), *The Fabric of Geology,* Addison-Wesley, Reading, MA, 24–48.
Strahler, A. N. 1952. 'The dynamic basis of geomorphology', *Geological Society of America Bulletin,* **63**, 923–938.
Strahler, A. N. 1992. 'Quantitative/dynamic geomorphology at Columbia 1945–60: a retrospective', *Progress in Physical Geography,* **16**, 65–84.
Sugden, D. E. (in press). 'The East Antarctic Ice Sheet: unstable ice or unstable ideas?' *Transactions of the Institute of British Geographers,* **NS 21**.
Watson, J. D. and Crick, F. H. C. 1953. 'The molecular structure of nucleic acids', *Nature,* **171**, 737–738.
Wolman, M. G. and Miller, J. P. 1960. 'Magnitude and frequency of forces in geomorphic processes', *Journal of Geology,* **68**, 54–74.